may therefore be of interest. There are two sets of data. In one the density and atomic weight of lead from thorite have been compared with the values of ordinary lead, and in the other a similar comparison has been made for the lead derived from two uranium minerals. These two sets, of course, cannot be compared together, as the densities of specimens are comparable only when they have been prepared under identical conditions. With due attention to this point the relative densities are, in the case of lead, capable of determination to a very high degree of accuracy.

In the first set of data (NATURE, February 4, 1915) the density determinations agreed in the case of three determinations on 73 grams of ordinary lead to within eight units, and in the case of two determinations on 65 grams of thorite lead to within four units in the fourth place of decimals. The first two values of the atomic weights in the following table are single determinations by a modification of Stas's method, the lead being converted into chloride, *via* the nitrate, in a quartz vessel without transference, and the ratio Pb : $PbCl_2$ determined. The third value is that obtained by O. Hönigschmid in Vienna on another fraction of the same thorite lead used in the density determination by the silver titration method from four determinations of the ratio $PbCl_2$: 2Ag and four of $PbCl_2$: 2AgCl, and the probable error is given as ±0·014 (*Zeitsch. Elektrochem.*, 1917, vol. xxiii., p. 161). The second set of data is that of T. W. Richards and C. Wadsworth (Journ. Amer. Chem. Soc., 1916, vol. xxxviii., pp. 221 and 1658). The atomic weights are also by the silver titration method. The value 207·20 for the atomic weight of ordinary lead has also been obtained by G. P. Baxter and F. L. Grover (Journ. Amer. Chem. Soc., 1915, vol. xxxvii., p. 1027), and the value 207·18 by O. Hönigschmid and Mlle. S. Horovitz (*Monatsh.*, 1915, vol. xxxvi., p. 355) by similar methods. (Compare also Ann. Rep. Chem. Soc., 1916, vol. xiii., p. 247.)

Variety of lead.	Atomic weight.	Density at 20°.	Atomic volume.	Difference from mean
Ordinary	207·199	11·3465	18·2619	+0·0009
Ceylon thorite... ...	207·694	11·3760	18·2572	-0·0038
	207·77		18·2639	+0·0029
		Mean	18·2610	
Ordinary	207·20	11·337	18·2765	-0·0026
Australian uranium ore	206·34	11·288	18·2796	+0·0005
Norwegian cleveite ...	206·085	11·273	18·2813	+0·0022
		Mean	18·2791	

The differences in the atomic volume are thus exceedingly small, and, moreover, they are not systematic. Rejecting the single determination of the atomic weight of thorite lead, it appears that ordinary lead with the intermediate atomic weight has an atomic volume slightly below that of the others. It seems quite safe to conclude that the atomic volumes cannot differ by so much as three parts in ten thousand and the atomic diameters by so much as one part in ten thousand. FREDERICK SODDY.

Relativity and the Velocity of Light.

IN his article in NATURE of February 17 on the general physical theory of relativity Mr. J. H. Jeans refers to recent experiments of Majorana, and his remarks imply that these experiments rendered it "possible to watch the progress of the ripple directly" and to measure the velocity of light in its unidirectional course from source to receiver, with the result that this velocity was shown to be constant. He contrasts these experiments with the original experiments of Michelson and Morley, in which the mean velocity of light in its outward and return journey after its reflection from a mirror was dealt with. As the point in question is a fundamental one, and as a statement to this effect has been made before, I think the matter should not be passed over.

The experiments of Majorana referred to are doubtless those described in *Comptes rendus* (No. 14, tome clxv., 1917, and No. 2, tome clxvii., 1918) designed to show the constancy of the velocity of light relative to the observer when reflected by a moving mirror or when issuing from a moving source. I venture to suggest that these experiments do not bear the interpretation that Mr. Jeans puts upon them, and that the experiment has not yet been devised that will enable a comparison to be made between the velocity of light on its outward and return journeys along the same path, or that will give a measure of the velocity on a single journey. The author of these papers makes no claim to have done this. I fear such an experiment is impossible.

C. O. BARTRUM.

32 Willoughby Road, Hampstead,
February 24.

I HAD not intended to make the statement which Mr. Bartrum considers is implied in my words, and am sorry that in aiming at brevity I appear to have achieved only ambiguity. It need scarcely be said that I agree that no experiment has been, or can be, devised which can measure the velocity of light in any unidirectional course. The impossibility of any such experiment is, in effect, the primary postulate of the theory of relativity.

It is, nevertheless, possible to compare two velocities along the same unidirectional course, and this is what Prof. Majorana claims to have done.

The Michelson-Morley experiment gave us the sum only of the times of two separate journeys—from A (light) to B (mirror) and back from B to A. We cannot even speak of comparing the time on AB with that on BA until we have defined time at B in terms of the time at A. If this is defined in terms of the relativity relation $t' = \beta(t - ux/c^2)$, then the Michelson-Morley experiment is consistent with the two journeys being performed with the same velocity c, and therefore in equal times, but it does not of itself establish equality either of velocity or of time. The additional information provided by the experiments of Majorana does, I believe, enable this equality to be proved.

Consider the problem in terms of an æther and a **FitzGerald-Lorentz** contraction. According to the Michelson-Morley experiment, the time on the double journey is equal to

$$l_0\left(1 - \frac{u^2}{c^2}\right)^{\frac{1}{2}}\left[\frac{1}{c-u} + \frac{1}{c+u}\right] \quad . \quad . \quad . \quad (1)$$

but there is so far no justification for identifying the two terms in this sum with the times of the separate journeys. The distributed expression for the time of the double journey might, in general, be of the form

$$l_0\left(1 - \frac{u^2}{c^2}\right)^{\frac{1}{2}}\left[\frac{1}{c-u+\alpha} + \frac{1}{c+u+\beta}\right] \quad . \quad . \quad (2)$$

where $c+\alpha$, $c+\beta$ are the velocities through the æther on the two journeys. For this to conform to the results of the Michelson-Morley experiment, expressions (1) and (2) must be equal, requiring that

$$\frac{2c+\alpha+\beta}{(c-u+\alpha)(c+u+\beta)} = \frac{2c}{c^2-u^2} \quad . \quad . \quad . \quad (3)$$

Now impose a further velocity v on the whole Michelson-Morley apparatus, so that its velocity through the æther becomes $u+v$. The first result of Majorana (*Phil. Mag.*, vol. xxxv., p. 173) shows

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that β remains unchanged. His second result (*Phil. Mag.*, vol. xxxvii., p. 149) shows that α remains unchanged. The time of the double journey is accordingly obtained by replacing u by $u+v$ in expression (2), and the Michelson-Morley result requires that equation (3) shall remain true when $u+v$ replaces u. Since Majorana's results held over a considerable range of values of v, it appears that (3) must be true for a whole range of values of u, requiring at once $\alpha=\beta=0$, so that the two terms in expression (1) must represent separately the times of the inward and outward journeys.

Translate this into relativity language, and it appears that when x, t, x', t' are related by the usual Lorentz transformation, then the Michelson-Morley experiment, when supplemented by the observations of Majorana, shows that both on the outward and on the inward journey light travels with the same constant velocity c.

J. H. JEANS.

Relativity and the Deviation of Spectral Lines.

THE prediction of the Einstein spectral-line effect rests on two assumptions, namely, (1) the radiating source behaves as a natural clock, and (2) the time-period of the source is transmitted by the radiation to the observer.

An alternative to the second of these assumptions is that the radiation transmits the Einstein interval ds rather than the time interval dt of a vibration. This alternative appears to be more in accordance with the general ideas of relativity.

Consider two light pulses leaving A at times t_A, t_A+dt_A, and arriving at B at t_B, t_B+dt_B. Since $ds=0$ along the world line of each pulse, it appears that the interval $\gamma_A dt_A$ between the two departures from A is equal to the interval $\gamma_B dt_B$ between the arrivals at B—that is, the Einstein interval, and not the time interval, is transmitted.

If this contention be correct, the Einstein effect should arise, not from the transference of the source, but from the transference of the observer to a different gravitational field.

It may be contended that the use of the principle of least time in the ordinary method of deducing the deviation of a beam by a gravitational field presupposes an underlying constant time period in the radiation. To this I would reply that it is possible to deduce the deviation without any reference to pre-Einstein physics. I propose to deal with this point in a communication to the *Philosophical Magazine*.

H. J. PRIESTLEY.

University of Queensland, Brisbane,
January 15.

Amplifying the Optophone.

MR. CAMPBELL SWINTON'S forecast in NATURE of March 3, p. 8, has been fully verified since he wrote. On Tuesday, March 1, the Marconi Co. kindly lent me one of their three-valve amplifiers working with an S. G. Brown loud-speaking telephone and wooden trumpet. Mr. F. Swann, of the Marconi Co., personally superintended the installation, and we succeeded without much difficulty in producing a sound which made ordinary printed matter "legible" to several blind pupils in a room. A reading demonstration from the amplified sound was given in the presence of Sir William Collins and Mr. C. P. MacCarthy.

This new development marks a great step forward, and I consider that Mr. MacCarthy and Mr. Campbell Swinton deserve credit for their initiative in this matter.

E. E. FOURNIER D'ALBE.

10 St. James's Terrace, N.W.8, March 5.

WITH reference to the letter on the above subject in NATURE of March 3, it may be of interest to record that thermionic amplifiers were applied to the optophone a considerable time ago with the object of increasing the sound in the ordinary telephone, but although it was evident that the sound could be amplified, it was decided that to increase the cost and complexity of the instrument to the extent involved by the addition of an amplifying set was not justified.

In 1919 Messrs. Barr and Stroud, Ltd., applied to Messrs. Marconi, of Chelmsford, who were kind enough to prepare a special amplifying set.

At a later date, through the kindness of Major Henrici, the valuable advice and assistance of officers of the Signal Department at Woolwich were also obtained.

Mr. Swinton, by the application to the subject of his great experience of amplifiers, has attained most encouraging results in making the optophone notes audible for instructional purposes.

JAMES WEIR FRENCH,
Director, Barr and Stroud, Ltd.

Anniesland, Glasgow, March 7.

The Peltier Effect and Low-temperature Research.

I WAS much interested to see Mr. A. A. Campbell Swinton's letter to NATURE of February 24, p. 828, on the above subject. So far as I am aware, the first suggestion to attain low temperatures by means of the Peltier effect was made by me when a student some twenty years ago. If Mr. Campbell Swinton will look up NATURE of August 15, 1901, p. 376, and also the *Chemical News*, 1901, vol. lxxxiv., p. 73, he will see an article by myself entitled "On a Possible Method of Obtaining the Absolute Zero of Temperature," in which the method is suggested in detail. There is little doubt that a great field of research would open out once the absolute zero of temperature were obtained, and temperature as a phase vanished from matter.

Attention may also be directed to a paper by Mr. Brinkworth and myself entitled "On the Heatless Condition of Matter" in *Chemical News*, 1902, vol. lxxxv., p. 194. Of course, it must be recollected that we were writing twenty years ago, long before modern developments occurred. Unfortunately, I have been out of touch with such matters, and have not had the opportunity of seeing whether any researches have been carried out on these lines owing to my work developing in a different direction.

GEOFFREY MARTIN.

109 Corporation Street, Manchester,
March 2.

WITH reference to Dr. Geoffrey Martin's interesting letter, it was because I thought it very possible that the idea was not new that I put my suggestion in the form of an inquiry. I have looked up his several most suggestive papers, which fully bear out what he says.

As I have pointed out, since the date of Dr. Martin's communications to NATURE and to the *Chemical News* in 1901, Prof. Kamerlingh Onnes has verified the disappearance of electrical resistance at very low temperatures which Dr. Martin, amongst others, predicted. It does not appear certain whether at such temperatures, when electrical conductivity in metals becomes infinite, either the Peltier effect or the corresponding opposite thermopile effect would operate.

Perhaps these effects may be enhanced, but possibly they may disappear; much would appear to depend upon how these low temperatures affect heat conduc-

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